

ART. II. *Experiments and Observations on the Endosmose and Exosmose of Gases, and the Relation of these Phenomena with the Function of Respiration.* By EDWIN D. FAUST, M. D. of Columbia, S. C.

THE remarkable phenomena discovered by M. DUTROCHET on the endosmose and exosmose of *liquids*, and the investigations of Dr. TOGNC, of Philadelphia, who repeated the experiments of the former eminent physiologist, and added some new ones to those previously performed in France, have been already placed before the readers of this Journal.* Both of these gentlemen have shown, that some of the important processes of the animal system hitherto referred to the agency of the vital principle, will, in all probability, be elucidated by the investigations in which they have engaged. They have even gone further, and applied the results of their observations with some success to therapeutics. Being convinced that the facts were destined to become useful in their application, and circumstances having prevented us from perusing the works of Dutrochet, we were about commencing a series of analogous experiments, when our attention was turned to another subject by a short article in the May number of the present Journal, (p. 217,) in which it was announced that Mr. GRAHAM of Glasgow had discovered two instances of the above phenomena, in gases. In the first, a bladder loosely filled with coal gas, being brought into a receiver of carbonic acid, was fully dilated, in twelve hours, by the entrance of the latter, while a little coal gas had exuded. In the second, common air being substituted for the coal gas, the bladder was again fully inflated.

These being the only experiments stated to have been performed by Mr. Graham, we have instituted, and are now pursuing a series, in which we have examined the relations of various gases, most of which have furnished strong examples of endosmose or exosmose, according to circumstances. We have satisfied ourselves that the principle acts generally, if not universally among gases, and have reason to hope that the results of future experiments on the subject, will throw light on some pathological facts not hitherto explained.

It is principally to the illustration of the function of respiration, however, that we would invite the attention of our readers, who will, we hope, feel interested, in following us through our application of the preceding phenomena, to the various circumstances of that pro-

* Vol. I. p. 423, Vol. III. p. 194, and Vol. IV. p. 73.

cess, on the chemistry and physiology of which philosophers are so much at variance. We hope to clear up some doubtful points, and to reconcile some previously conflicting authorities. Before proceeding however to any other observations, we think it proper to state some of the opinions and experiments of others, and a few of our own, on the chemical changes produced in air by the blood.

It is allowed by all authorities, that air, when returned from the lungs, contains an increased proportion of carbonic acid, and a smaller proportion of oxygen, than air which has not been respired. Whether the quantity of oxygen lost is exactly equal to that acquired in the form of carbonic acid, is a disputed point. The experiments of Dr. PRIESTLEY led him to the conclusion that the carbonic acid evolved did not compensate for the oxygen consumed.* The same result according to Dr. BOSTOCK,† and others, was obtained by LAVOISIER, in experiments on the Guinea pig; and THOMSON states‡ that in experiments subsequently made by M. LAVOISIER, and of which, M. SEGUIN was the subject, the quantity of carbonic acid was diminished by nearly one-half.§ In 1806, Mr. DALTON, from a number of experiments concluded that the carbonic acid formed, exactly compensates for the oxygen consumed.|| Dr. Thomson conducted experiments which indicated a loss of oxygen; but this loss was so variable, that he was disposed to refer it to something else than respiration.¶ Here he was wrong.

Several chemical authors would induce us to suppose that the evidence of Sir H. DAVY is the same as that of Mr. Dalton, but on examining his experiments,** it will be found that they confirm those of Priestley and Lavoisier, indicating a decided loss, (absorption,) of oxygen. The celebrated experiments of ALLEN and PEYST†† led them to the conclusion that the carbonic acid exactly compensates for the oxygen consumed; and after this, the doctrine was generally adopted, until the more recent researches of EDWARDS,‡‡ which confirm

* On air, MDCCXC. Vol. III. p. 379.

† Physiology, 1826, Vol. II. p. 94.

‡ Thomson's System, Cooper's edition, 1818, Vol. IV. p. 467.

§ The first experiments, however, are much more to be relied on, though all lead to the same conclusion.

|| Thomson's System, 1818, Vol. IV. p. 468.

¶ Idem.

** Researches on Nitrous Oxide, &c. 1800, p. 434 to 438 inclusive.

†† Philos. Trans. for 1808, p. 279.

‡‡ De l'Influence des Agens Physiques sur la Vie; we shall take the liberty of quoting Edwards on the authority of Bostock, Thenard, and Turner, as the original is not in our possession.

those of Priestley. Dr. PROUT has ascertained, according to Dr. Thomson* and others, that the quantity of carbonic acid varies at different hours of the day. The discordant results of these philosophers have at length been reconciled by Dr. Edwards, who, by numerous and very accurate experiments, has proved, that the quantity of oxygen consumed, and of carbonic acid formed, varies in different animals and at different times; and that while at times the loss of oxygen is almost nothing, it sometimes amounts to the large quantity of more than one-third of the volume of the carbonic acid produced.† The fact then that a considerable quantity of oxygen is usually but not always absorbed, and not returned, may be considered as established.

The question whether the absolute volume of air is diminished by respiration, is answered affirmatively by those experiments of Dr. Edwards, in which a positive loss of oxygen is proved. Other authorities to the same effect are, according to Dr. BOSTOCK, (some of the original works, not in our possession, being of course accurately quoted by him,) MAYOW,‡ HALES,§ DAVY,|| LAVOISIER,¶ GOODWYN,** BERTHOLLET,†† JURINE,‡‡ and SPALLANZANI.§§ CRAWFORD, in his work on animal heat, p. 146, states, that animals confined in air, over mercury do not diminish it.|||| But in the same page he states, that if the air be over water, it loses nearly one-fifth its volume, and therefore nearly the whole volume of its oxygen. Now as we know that an animal dies in air long before all the oxygen is consumed, it ought to follow, that, in the above case, a portion of nitrogen was lost. This must be considered when we come to the subject of nitrogen. Crawford must have been mistaken when he supposed the residual gas to consist almost entirely of nitrogen. Dr. Edwards has shown here, however, as before stated, that there is almost always a diminution, which varies according to circumstances.¶¶ The experiments of Allen and Pepys do not overthrow this conclusion, since from their nature they could not be so accurate as those of Edwards.

* System, Vol. IV. p. 469; also Bostock's Physiology, Vol. II. p. 87.

† Bostock's Phys. Vol. II. p. 97.

‡ Tract, p. 105.

§ Stat. Essays, Vol. II. p. 238, 320.

|| Researches, p. 431.

¶ Mem. Acad. Scien. pour 1780, p. 401; Ann. Chim. t. v. p. 261; Mem. Soc. Roy. Med. pour 1782-3, p. 572.

** Connexion of Life, &c. p. 51.

†† Mem. Soc. D'Accueil, t. II. p. 454, 463.

‡‡ Mem. Soc. Roy. Med. t. x. p. 25.

§§ Mem. sur la Respir. p. 102.

|||| Priestley and Boyle state the same fact.

¶¶ Bostock's Physiology, Vol. II. p. 101.

We come now to the important question whether nitrogen is absorbed in respiration. Lavoisier asserts in the *Mem. Acad. Scien.* pour 1777, p. 193,* that no nitrogen is absorbed, and he is supported by Allen and Pepys.† Priestley, however, found a distinct absorption of nitrogen,‡ and his opinion is confirmed by the experiments of Sir H. Davy.§ CUVIER says of the air in breathing, “il s’y perd un peu d’azote,”|| but does not here state any experiments. Bostock¶ quotes Dr. HENDERSON and Professor PFAFF, to the same effect. But physiologists were again puzzled by the researches of NYSTEN, BERTHOLLET, and DESPRETZ, which pointed out a positive increase, instead of a diminution of the nitrogen.** All these contradictions have, like the preceding, been reconciled by Dr. Edwards, who, by experiments made in the most accurate and careful manner, has proved, that there is sometimes a loss, sometimes a gain of nitrogen, while sometimes the change is very slight.†† We can find, however, no reason for his conjecture, that a simultaneous absorption and exhalation of nitrogen is always going on, and that the result depends upon the predominance of one or the other of these processes. Having thus stated the three principal points in the chemical process of respiration, we shall here give from Thenard‡‡ the results of Edwards’s researches, adopting the opinions as our own, with the exception of the last article, and of the words “ou en partie.” As regards the last article, we are satisfied, that whatever nitrogen enters the blood, goes to the uses of the system; and not that it is expelled from the blood merely to give place to another supply of the same gas. “L’oxigène qui disparaît dans la respiration de l’air atmosphérique est absorbé en entier; il est ensuite porté en tout ou en partie dans le torrent de la circulation.

“Il est remplacé par une quantité plus ou moins semblable d’acide carbonique exhalé, qui provient en tout ou en partie de celui qui est contenu dans la masse du sang.

“En outre, l’animal respirant de l’air atmosphérique absorbe de l’azote; cet azote est porté en tout ou en partie dans la masse du sang.

“L’azote absorbé est remplacé par une quantité plus ou moins

* As quoted in Bostock’s *Physiology*, Vol. II. p. 102.

† *Phil. Trans.* for 1808, p. 264.

‡ On Air, MDCCXC. Vol. III. p. 380.

§ *Researches*, p. 434.

|| *Leçons d’Anatomie Comparée*, Paris, An. xiv. 1805, tom. IV. p. 303.

¶ *Physiol.* Vol. II. p. 103.

** *Turner’s Chemistry*, 1828, p. 432.

†† Bostock’s *Phys.* Vol. II. p. 105.

‡‡ *Traité de Chimie*, 1827, tom. IV. p. 561.

équivalente d'azote exhalé, qui provient en tout ou en partie du sang."

Thus much for the present on respiration.

Air is acted upon by blood which has been removed from the veins. In this case, a portion of its oxygen is removed, and it is found to contain carbonic acid.* Whether any nitrogen is absorbed, we consider as uncertain; for though Thomson and some others state that no absorption was perceptible,† we must recollect that these gentlemen did not detect the same phenomenon in respiration, in which process we know it does take place. The question, however, is not important in this place.

Among the changes which venous blood undergoes in becoming arterial, the most remarkable is the red colour which it acquires. After all the objections against the opinion,‡ we are still inclined to consider the black colour of venous blood as due to the presence of an excess of carbon, free, or loosely combined. It will not redden without the removal of this carbon, or its more intimate union with the other constituents of the blood. Dr. Priestley found that arterial blood blackened when placed in vacuo.§ This has been supposed to show that the blackness is independent of carbon; but it by no means does so. A change has plainly occurred in the blood, by which the former arrangement of its constituents is destroyed. What fact goes to show that this change does not consist in the development of carbon? None! while all the collateral experiments and observations tend strongly to show that such is the case. The same chemist discovered that arterial blood is slowly darkened by nitrogen.|| Here, as before, the observations just made are perfectly applicable. According to Dr. Thomson,¶ Fourcroy obtained the same result from hydrogen. The above remarks apply again, but in addition, Dr. Edwards has shown that blood absorbs hydrogen;** and here it is self-evident that the hydrogen has united with oxygen, nitrogen, or carbon, or with two or more of them; hence, in the chemical revolution which occurs, a portion of carbon is no doubt set free or loosely combined. Dr. Thomson states in the page last quoted, and on the authority of Fourcroy, that arterial blood left in oxygen gradually

* Thomson's System of Chemistry, 1818, Philadelphia, Vol. IV. p. 473.

† Idem.

‡ Ellis's inquiry into the changes induced on air by germination, vegetation, and respiration, 1807, p. 184 and 196.

§ On Air, Vol. III. p. 364.

|| Idem, p. 363.

¶ System, Vol. IV. p. 474.

** Turner's Chemistry, 1828, p. 434.

darkens: and that after this change, oxygen will not redden it. This we consider very interesting, as indicating that the spontaneous changes of blood may develop carbon, in such a form, that it will not unite with oxygen at common temperatures.

Physiologists, who are chemists, but not enthusiastically so, will find constant use for their knowledge, without being led astray by it. But to adopt the view least favourable to our opinions, and to grant that under extraordinary circumstances, some secret cause, independent of carbon, may blacken the blood; every sound reasoner will perceive that this will not affect the conclusion, that under ordinary circumstances, venous blood owes its colour to carbon. The introduction of oxygen and nitrogen into the blood is no doubt intended for the more perfect animalization of that fluid; which has been deteriorated in the capillary system. The lymph and the chyle are poured into the venous blood, just before it is sent to the lungs; and a very fair inference is, that this is done, in order that these fluids, and especially the latter, may receive those changes by which one or both are to be converted to fibrin, a substance more advanced in the scale of vitality. The fibrin of the venous blood, too, has plainly degenerated from its arterial state, and lost a portion of its contractility.* Hence we see that the venous fibrin, the lymph, and the chyle, and probably also the other constituents of the blood, require chemical changes, to effect which, the introduction of oxygen and nitrogen, and the escape of carbon are necessary. This view will explain fully the facts ascertained by Dr. Edwards. If the nature of the animal or the food used, or the operation of any other cause, have yielded the blood an excess of carbon, a proportionate quantity of oxygen will be spent on that substance; and if, at the same time, the blood contain hydrogen loosely combined, or some animal matter deficient in oxygen, a portion of that gas will be detained, and the carbonic acid formed will not compensate for the oxygen absorbed, but as the nature of the blood varies with that of the animal, the food, &c. so will the quantity of oxygen lost vary. In like manner, the nitrogen, if the above circumstances have furnished it in greater quantity than the purposes of the system require, will be exhaled from the blood; and we shall show that this exhalation is an instance of exosmose. If, on the contrary, any of the proximate principles of the blood be very deficient in nitrogen, a portion of that gas will be absorbed, and we shall show how this is effected by, and agreeable to, the laws of endosmose. The blood of the veins, when directly exposed to the

* Broussais's Physiology, 1826, p. 261.

air, loses carbon, and is reddened, as in the lungs. We have not yet discovered the more intimate changes which occur in its mass. We shall now proceed to notice some experiments on the endosmose and exosmose of gases.

It results from the experiments of Mr. Graham already stated, and from several of our own, that when two gases are separated by a membrane, *both of them pass through the partition*; but as one of these motions predominates over the other, the result is endosmose or exosmose. The membrane, (if we may so call it,) used in most of the following experiments, was the crop of the common domestic fowl. We have obtained similar results from portions of bladder.

July 21st. 2 o'clock, P. M. thermometer 88° Fahrenheit. A membrane filled moderately with nitrogen,* was placed in a receiver of hydrogen over water. The membrane was suspended from a copper wire rested between it and the side of the receiver, and was so nearly in contact with the wire, that its slightest expansion or contraction could be detected. In twenty hours, (therm. 82°) there had occurred a considerable endosmose.

July 23d. 12 o'clock, therm. 88° . The above experiment was reversed. In twenty hours, (therm. 82° ,) there was a very considerable exosmose, the membrane being quite flaccid.

July 25th. 12 o'clock at night, therm. 84° . A membrane moderately full of common air, was placed in carbonic acid. In half an hour the membrane was fully dilated, and on examination in eight hours, (therm. 84° ,) was found to have burst.†

July 26th. 8 o'clock, A. M. therm. 82° . The last experiment being reversed, the effect also was reversed; exosmose occurring.

July 27th. 12 o'clock, thermometer varying very little from its previous state. A membrane of hydrogen being introduced into carbonic acid, expanded sensibly in a few minutes, and in two hours the endosmose was found to have burst it.

August 21st. 11 o'clock, A. M. therm. 83° . A ridge of sealing-wax having been previously fixed on the outside, and near the wide part of a glass funnel capable of containing two ounces of air by measure, a piece of bladder was tightly tied over the wide part of the funnel, thus forming a membranous septum between any gas in the

* The nitrogen used in our experiments was obtained by the slow action of phosphorus on common air, and therefore contained a little phosphorus. This, however, we separated, by the cautious addition of a minute quantity of chlorine. The hydrogen was obtained from weak sulphuric acid and iron.

† Oxygen and nitrogen separately give the same result; and the endosmose would probably as above, burst the membrane in some hours.

funnel and that outside of it. The funnel was now filled with nitrogen, and put under a receiver full of oxygen, in such a position that any gas passing from its point would be caught in a small receiver. In two hours the thermometer rose four degrees, and half a drachm of gas passed from the point of the funnel. We are not sure, however, that this was the result of endosmose, for not an atom escaped after the thermometer reached 87° . Hence, we may attribute the slight escape of gas, principally, if not wholly, to increase of temperature, and a slight contraction of the bladder in becoming partially dry. As, however, the funnel remained precisely full, at the end of twelve hours, at which time the thermometer had regained its original temperature, it should follow that half a drachm of oxygen must have entered the funnel. But on analyzing the contents of the funnel, it appeared that they had received a drachm and a half very nearly of oxygen; to make room for which only one-third its volume of gas had passed from the point of the funnel. Hence about one drachm of nitrogen must have left the funnel, by exosmose through the membrane. Of course then there was a double current, in which the endosmose and exosmose were very nearly equal. We repeated this experiment, with various modifications and precautions, before obtaining a satisfactory result.

July 31st. 12 o'clock, therm. 90° . A membrane filled with common air, (the pressure of which from within outwards was slightly increased by a column of water about three inches high,) was placed under a receiver of common air. At first there was a slight expansion, because the membrane, being wet, was cooled by evaporation, when in the open air, but when put under the receiver, the air regained its original temperature and volume. After this, there was a very slow escape of air through the membrane, by no means comparable to the slowest instance of exosmose yet observed. We thus see, that gases escape very slowly, through moist membranes, even in their æriform state; much less then will they be able to escape, when deprived of their elasticity by solution in the blood. This last reflexion will oblige us to admit one of two things; viz. either that carbon and nitrogen are thrown into the air-cells in a liquid form, and then become gaseous, or that they are thrown out by the influence of exosmose in a gaseous form, the former united with oxygen. But as the former of these opinions has not a single fact, or even analogy to support it, it must be we think rejected, while the latter view is confirmed by many facts; among which one may be here stated, viz. that the change of blood and escape of gas occur, when the blood is contained in a bladder, and is, therefore not under that vi-

tal influence by which the carbon might be imagined to be secreted in a liquid form, and thus reach the air.

August 18th. Half after 6 o'clock, P. M. therm. 84° . A beef's bladder was filled with blood drawn from *the same animal* two hours before, and which had been carefully preserved from contact with air, by being kept in a large bottle perfectly filled, and closed with a ground glass stopper. The external surface of the bladder being washed, and wiped with a dry cloth, the whole was placed in the bottom of a large glass jar, which at one side was prevented from contact with the bladder, by the interposition of a piece of wood, thus allowing a circulation of air from the bottom to the top of the jar. A small jar half filled with lime water, was now placed upon the bladder, and the external air was completely excluded by closely covering the mouth of the large jar. On examination at the end of forty minutes, a pellicle of carbonate of lime had formed on the surface of the lime water. The density of this pellicle continued to increase until eleven o'clock, when, being broken by slight agitation, the greater portion precipitated, a little still floating. At twelve o'clock the pellicle having again become complete, was again broken and precipitated by agitation, and the process was left undisturbed until morning. At this time a considerable quantity of carbonate of lime having been formed, the process was stopped. The formation of carbonate of lime in the vessel exposed over the bladder of blood, was beyond all comparison, more rapid than in another jar of lime water, exposed to the open air. Now if the great thickness and density of texture of a beef's bladder just taken from the animal, be compared with the extreme tenuity of the tissue of the pulmonary capillaries, if the free circulation of liquid blood in the lungs, be compared with the motionless and solid coagulum in the bladder, finally, if the high temperature of the lungs be considered, there will, we think, not be any doubt of the absorption of oxygen, and the escape of carbonic acid. Nor must we forget the immense extent of the respiratory surface, and the constant renewal of the air, in opposition to the small surface of the bladder, (half of which touched the glass and not the air,) and the unrenewed and stagnant air of the jar. If then the oxidation and decarbonization of the blood occur under the least propitious circumstances, how much more rapidly will they proceed when every condition conspires to promote the action. As, too, these changes proceed in a dead membrane, they are but partially, if at all, dependent upon vitality, and result principally or wholly from the agency of those physical influences which form the subject of this paper. It must be stated, that *during fourteen hours* occupied in the above experiments,

half an ounce of red serum escaped through the coats of the bladder. But that this was not the source of the carbonic acid, is shown by the small quantity of the serum, and by the fact, that the formation of carbonate of lime was considerable before the escape of serum commenced. My experiment was partially performed by Dr. Priestley, who found that blood contained in a bladder was rapidly reddened, wherever it touched the bladder,* and that the layer so reddened, was about as thick as if the contact with air had been immediate. He did not, however, attempt to ascertain the escape of carbonic acid from the bladder. Professor GORHAM states, that both the reddening of blood, and the escape of carbonic acid, have been found to occur, through animal membrane,† as in our own experiment. As, however, he refers to no authority, we concluded to repeat the process as above detailed.

Having now stated those facts which most immediately influence the subject before us, we shall proceed to apply them to the explanation of some vital phenomena. It will, from the experiment of July 26th, and the corresponding note, appear, that when carbonic acid is separated from common air, or oxygen, or nitrogen, by a membrane, the acid passes into the air. This corresponds perfectly with the function of respiration, in which the blood being on one side of the membrane, and air on the other, the carbonic acid passes into the air. There is no power, we think, but exosmose, which can produce this effect, for though *gaseous fluids* do, under the influence of pressure, *very slowly* escape from membranes, yet it is plain, at the first glance, that when a gas has, like the carbonic acid of the blood, lost its elasticity by becoming liquid, it has no longer this power. It also appears, not only that no known vital power can produce the phenomena, but that they occur in dead membranes, and therefore removed from the influence of vitality. We are hence forced to examine the laws of inanimate nature, for a clue to this secret, and we find it fully revealed in our experiments of July 26th, and August 18th.

In conformity with the existence of a double current, we might expect a tendency in both the oxygen and nitrogen of the atmosphere, to enter the blood.‡ Accordingly, as we have already shown, both of these gases do often, if not always, enter the circulation. The reader may recur to our quotation from Dr. Edwards. It is easy to see

* On Air, Birmingham, MDCCXC. Vol. III. p. 369.

† Elements of Chemical Science, Boston, 1820, Vol. II. p. 448.

‡ We find, by experiment, that this double current does occur in this, as in other cases.

what circumstance limits the introduction of these gases into the blood. As there is no room in the pulmonary capillaries for gaseous matter, the oxygen and nitrogen will assume the liquid form, in entering the circulation,* and no more can enter than the blood can chemically unite with. Hence, if the blood contain oxygen enough for the purposes of the system, the carbonic acid evolved will compensate for the oxygen consumed; while, if the circulating mass be deficient in that principle, the considerable quantity absorbed by endosmose, will not be compensated for by the exosmose of carbonic acid, but a portion of oxygen will be spent upon the tissues and secretions. All this is in exact conformity with facts already stated, and which, we think, can be explained so as to reconcile conflicting authorities, on no other principles than those of endosmose and exosmose. In like manner, if the blood be exactly saturated with nitrogen, so that it can contain no more in a liquid state, it will be impossible for any atmospheric nitrogen to enter the circulation; while, if any of the proximate principles of the blood be deficient in nitrogen, the atmosphere will lose a portion of that substance, as we have shown does actually occur in some cases. We come now to show how, as sometimes happens, there may be an actual exosmose, instead of an absorption, or a neutral state of nitrogen. We have shown, by our experiment of August 21st, that when oxygen and nitrogen are separated by a membrane, both endosmose and exosmose occur; oxygen passing into the nitrogen and nitrogen into the oxygen. Hence, in respiration, as the oxygen of the air is on the outside of the membrane, and blood on the inside, if that blood has been supplied by the function of digestion, with more nitrogen than the system needs, nitrogen will be evolved, and the respired air will receive an addition of that gas. We have found by experiment, that the carbonic acid of the air-cells tends to produce a similar exosmose of nitrogen.† There is no difficulty, then, in explaining why nitrogen is sometimes absorbed, at other times exhaled, and why, in some cases, neither change occurs. This last takes place when the blood has neither excess nor deficiency of nitrogen. Dr. Thomson states, on the authority of Allen and Pepys, that even under ordinary circumstances, if pure oxygen be respired, a considerable exhalation of nitrogen takes place.‡ He adds, that this cannot be accounted for until

* This must be the case in extensive emphysema also; since the entrance of gaseous matter would prove fatal.

† An experiment of August 22d, in which nitrogen and carbonic acid being separated by a membrane, a double current occurred.

‡ System, Vol. IV. p. 470.

the composition of nitrogen is discovered. Our own discoveries on exosmose, will save chemists the necessity of prematurely forcing nitrogen into the catalogue of compounds.

“When an animal is made to breathe a mixture of 79 hydrogen gas, and 21 oxygen gas, the respiration goes on without any inconvenience; carbonic acid gas is formed as usual, and at the same time a portion of the oxygen disappears, and azote is found in its place.”* Without the agency of the powers which we have been considering, it will we think be quite impossible to explain the exhalation of carbonic acid and nitrogen into a mixture of oxygen and hydrogen. No other agent known to the physiologist, the natural philosopher, or the chemist, can clear up the mystery. From the facts which we have ascertained we hope to remove the obscurity altogether. It is evident that the evolution of carbonic acid is merely an exosmose, induced by the oxygen taken into the air-cells, as already explained in elucidating the respiration of common air. It will be seen too, from our experiment of July 27th, that the presence of hydrogen in the air-cells will produce an exosmose of carbonic acid. Our experiment of August 21st shows that the presence of oxygen in the air-cells will produce an exosmose of nitrogen from the blood. It will also be seen from our experiment of July 21st, and the general law immediately preceding it, that the presence of hydrogen in the air-cells, will, in like manner, cause the evolution of nitrogen from the blood. In this manner, the phenomena, otherwise incomprehensible, are rendered intelligible and even simple.

We extract from our paper on malaria, published in the number of this journal, for May 1830, the following observations. *According to Dr. Beddoes, the blood and muscles of animals killed by confinement in this gas, (carburetted hydrogen,) have a red colour, less distinct than that produced by oxygen, but very different from the dark colour exhibited by animals killed by drowning or carbonic acid. There is much obscurity in the reddening of blood by this gas, and there seems to be no error, as Dr. Beddoes speaks very positively of the facts, and mentions some decisive experiments.*

Dr. Thomson states that “carburetted hydrogen gas gives venous blood a fine red colour, a shade darker than oxygen gas does, as was first observed by Dr. Beddoes, and at the same time a small portion of the gas is absorbed.”†

From another page of our paper on malaria, we take the following remarks on carburetted hydrogen:—*Perhaps, indeed, its power of*

* Thomson's System, Vol. IV. p. 470.

† System, Vol. IV. p. 473.

producing internal accumulation of blood, will afford the only possible explanation of its effect of reddening the muscles of animals destroyed by it; for it is evident that the gas itself cannot decarbonize the blood. This, however, is not satisfactory to ourselves, for the venous blood is redder than in cases of death from carbonic acid; and it would thus seem that the gas interrupts those changes in the parenchymatous structure of the organs by which the arterial fluid is carbonized. The subject, we again confess, is involved in much mystery.

The difficulty, which was insurmountable when our paper was written, no longer exists at all, when late discoveries are applied to the investigation of the subject. It will be seen, from the experiment of Mr. Graham, related near the commencement of the present paper, that when carburetted hydrogen is separated by a membrane from carbonic acid, there is a strong and rapid movement of the latter into the former. Hence, when carburetted hydrogen is taken into the air-cells, there must be a decarbonization of the blood; an escape of carbonic acid from that liquid. Our original opinion, then, that this gas *cannot decarbonize the blood*, was incorrect. The reason, we conceive, why the blood is less reddened in this case than by oxygen, is, that the gas inspired, having no oxygen, the blood has no source for the acidification of its excess of carbon. Whatever portion of carbonic acid the blood may contain, will escape, and a portion of uncombined carbon may seize upon any oxygen loosely attached to the blood, and also escape. Hence the blood will be reddened. But as there is a scarcity of oxygen, a portion of carbon will still remain and the reddening will not be perfect.

We would now submit a question to our readers. Does the carbon of the venous blood exist as carbonic acid, or does it form a more complex union, and merely attract the oxygen of the blood *to itself exclusively*, when the disposition, (electrical?) to exosmose is elicited by the contact of air, or other gases, with the pulmonary surface? Iron will not take up the oxygen of water, until a new influence, perhaps electrical, is developed by the addition of sulphuric acid. Are the cases parallel?

Besides the illustrations above given, we have found, in examining a considerable number of works, during the preparation of this paper, many facts, which, otherwise obscure, are fully elucidated by the facts above detailed.

In concluding, we will advert for a moment to the endosmose and exosmose of *liquids*, and suggest to those who are pursuing that subject, the propriety of an attempt to ascertain whether the true com-

munication between the foetal and maternal vessels of the placenta be not a communication by endosmose and exosmose, and not by the direct communication, the mass of evidence against which is so strong, that in favour of it so limited.

Columbia, S. C. August 23d, 1830.

ART. III. *On the Penetrativeness of Fluids.* By J. K. MITCHELL, M. D. Lecturer on Medical Chemistry in the Philadelphia Medical Institute.

IN 1829, I read before the Philosophical Society, a short memoir on a new method of forming gum elastic into thin plates, sheets, and bags. In some instances the balloons formed by the process then described, had, when filled with hydrogen gas, the power of ascending to a considerable height in the atmosphere. Those which were confined to the atmosphere of my lecture room, at the Medical Institute, descended again after a period of time, varying from an hour to two days. The cause of the descent, which did not seem of easy explanation, became a subject of investigation.

The gas might have escaped from the balloons at the ligature, or by permeating the dense wall of gum elastic, or by uniting chemically with the internal surface of the latter. To free the gas from the compression to which it is subjected in a balloon, I confined it in a wide-mouthed bottle, over the aperture of which I tied very firmly a thin sheet of the elastic membrane. In a few hours the descent of the cover into the cavity of the bottle gave evidence of a diminution of the contained gas, and finally the cover was burst inwards by the pressure of the atmosphere, so great had been the rarefaction of that which remained in the bottle. On weighing the membranous cover, no gain in weight could be perceived, so that I presumed that the gas had escaped. By repeating the experiment, and covering the bottle with a small bell-glass holding atmospheric air, I found, after a time, in the latter vessel, an explosive mixture, while the contents of the bottle itself were found to be pure or nearly pure hydrogen. Evidence was thus afforded that hydrogen penetrated the membrane not by any *vis a tergo*, for no pressure was applied, but by some inherent power of considerable amount. The facility of permeation appeared also much greater in the hydrogen than in the atmospheric air, which, if it entered at all into the bottle, did not pe-